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LETTER FROM THE PRESIDENT'S DESK

Summer is a busy period for archaeologists connected with the Schools. Professors Reed, Richardson, Winnett and R. B. Y. Scott worked with Professor Pritchard at el-Jib. They were joined at various times by several members of the School staff and by students from the Church Divinity School of the Pacific. They report a very profitable season. Professor Cross of Harvard was in Jerusalem working in the "scrollery" and we hope to hear a progress report before too long.

I returned to Sardis this summer, where the Cornell-Harvard group worked for the second year. It was a very good season, and a fuller report from Director Hanfmann can be expected in the spring.

You will note on the final page of this issue that the Assistant Treasur r of the Schools is now the Chase Manhattan Bank of New York. Th s ends a long association with the Provident Tradesmens Bank and Tru t Company of Philadelphia, which dates from the time when the offic s of the Schools were in that city but has become less and less convenie t since our offices have been moved elsewhere. I should like to expre s our appreciation for the past services of the Provident Tradesmens Ban & Trust Company.

The work of the Schools was carried on during my absence thi summer by the First and Second Vice-Presidents, ably assisted by Mr Walton and Mrs. Otis, my secretary here in Ithaca. I deeply appreciat their services during my absence.

Finally, the transfer of authority in Jerusalem from Dr. Winnett to Dr. Pope took place smoothly with the help of Dr. Reed and severa other former staff members. May I wish the new staff good fortune for the coming year!

A. Henry Detweiler

October 5, 1959

AN AERIAL RECONNAISSANCE OF THE NEGEV

Nelson Glueck

A three-hour aerial reconnaissance over parts of the Negev [1] was made possible for me on March 13, 1959, through the courtesy of the Israel Defense Forces. The small Piper Cub plane was slow enough for comparatively leisurely observation. It was an excellent day for the flight. As a result of heavy rains and even snow during the preceding weeks, the land did not have the burnt-out appearance it assumes in summertime. Pools of water remained in many wadi-beds and in hollows of the wet brown earth, which for the nonce had absorbed as much as it could hold. In some instances, terrace plots in and alongside dry stream courses already had been or were being ploughed by semi-nomadic cultivators, and in others there could be seen the green of emerging crops. The strong stone terrace walls which in Judaean and Nabataean-Byzantine times had been erected to check the fury of the winter and spring freshets and force their waters to percolate into the soil and

[1] Cf. Bulletin 152, p. 18, n. 1, for a listing of reports of preceding seasons of archaeological exploration of the Negev, and *Rivers in the Desert*, published by Farrar, Straus, and Cudahy in January 1959, for a general account of the history of the Negev based on our archaeological discoveries there. The March, 1959, archaeological expedition was again carried out on behalf of the Hebrew Union College-Jewish Institute of Religion, under the auspices of the David W. Klau Foundation and with the assistance of a grant in aid from the American Department of State, under the Special Cultural Program for Israel.

r ain suspended there for many months to provide the necessary m isture for the wheat or other crops, could clearly be seen veining p ts of innumerable wadis, together with their related walls paralleling t banks on the slopes above them (Fig. 1).

We had established from examination on the ground how separate s etches of such terraced wadi-beds were contained within elongated

FIG. 1. Boxed sections of terraced wadi-beds, below site 171 (Iron II). (Photo Israeli Airforce)

sectioned boxes, formed by lateral walls on the slopes above their banks and closed by horizontal walls at either end of the wadi-bed, with shorter cross-walls subdividing the area in between into separate terraced fields.[2]

[2] BULLETIN 131, pp. 9-10; 138, pp. 17, 26; 149, pp. 13-14; 152, p. 23; *Rivers in the Desert*, pp. 216, 219 and Figs. 32, 55. This phenomenon of boxed sections of terraced wadis in the Negev has latterly also been noticed by others; cf. *Israel Exploration Journal*, 8: 4, 1958, pp. 242, 243, 245, 261, 262, 263. That this fashion of trapping rainwater in the soil of carefully terraced sections of dry wadi-beds, which were further protected by walls on the slopes above their lengths, was well known and intensively practiced in the period of the Judaean kingdom has been most clearly shown by Frank M. Cross, Jr., and J. T. Milik in their explorations and excavations in the isolated valley of the Buqê'ah in the northern part of the Judaean wilderness; cf. BULLETIN 142, pp. 5-15. As they have most convincingly pointed out, *op. cit.*, p. 15: "The happy circumstance that the dams (and terraced wadi-beds) of the Buqê'ah are associated with a single period of occupation (Iron II) in the region establishes their dating and furnishes, therefore, unique data for the history of irrigation and water conservation techniques before the Roman period in Palestine."

The walls on the slopes paralleling the sides of the wadis were designe to conduct the run-off water from the hillsides in carefully calculate fashion into the terraced fields or to lead it into cisterns and not to perm it to wash undesired rocks and soil on to the fields in the wadi-beds belov Numerous channels cut into the earth, frequently stone-banked, wer visible curving downward over connected hills, sometimes for miles, t collect and lead the water of occasional rains to the terraced fields prope or more often than not to carefully placed cisterns, whose total numbe in the Negev must mount up into the thousands. No Iron II fortres or village could have existed in the Negev without these cisterns o various types, the digging of which is Biblically attested.[3] Later, th numerous Nabataean and Byzantine settlements and their environs in the Negev were literally dotted with them.[4] The art of conserving water in cisterns, behind dams and in the soil itself was well advanced in the period of the Judaean Kingdom and brilliantly enhanced in the centuries of Nabataean and Byzantine flowering.

With striking clarity, we could see from the plane how sterile hilltops and slopes had been pressed into service as catchment areas to help collect as many drops of rainwater as possible. This was particularly clear in the case of the hilltop and slopes of Site 367, with which I have dealt previously.[5] The very low, rather oval wall surounding the hilltop of Site 367 is unmistakably clear when flying several hundred feet above it. It consists of small stones piled together to form a distinctive line of wall and is actually clearer from a plane than when seen on the ground. There it was only by walking all the way around that it was possible to see that it surrounded the top of the hill and could not possibly have served any purpose other than diverting the occasional rainwater to fixed channels which led in an intricate manner, in combination with continuous rows of pebbles, among which were rows of *teleilât el-ʿanab*, to terraced fields in the Wâdī Ḥafîr below.[6] This time it was possible to see from the air, that the rows of pebbles and of *teleilât el-ʿanab* on the hilltops and slopes by Qetsiot[7] (381), opposite Nitsanah, represented an extension of almost continuous fields of them reaching to site 367, located several miles to the south, southwest of it.

As a result of this reconnaissance flight, air photographs were again made available to me by the Israel Defense Forces; these show the entire relationship of the walled catchment basin of the top of the hill of Site 367 to the intricate patterns of rows of pebbles and of *teleilât el-ʿanab*

[3] II Chronicles 26: 10; BULLETIN 152, pp. 23, 28, 29, 33; "Meḥqārîm Arkeʾōlōgīyîm ban-Negev," in the Tel Aviv daily newspaper *Maʿarîv*, Sept. 14, 1958, pp. 22-23; *Rivers in the Desert*, pp. 172, 174, 222.

[4] *Rivers in the Desert*, pp. 222-225.

[5] BULLETIN 152, p. 21; 149, pp. 9-15; 145, pp. 19-20; *Rivers in the Desert*, pp. 220-222.

[6] BULLETIN 152, p. 21; *Rivers in the Desert*, p. 221; for the first correct explanation of the *teleilât el-ʿanab* and their associated rows of pebbles as means to help catch and lead rainwater from hillslopes to terraced fields alongside or in wadi-beds, cf. Tadmor, Evenari, Shanan and Hillel in *Ketavim* 8: 12 (1957), pp. 99-121.

[7] BULLETIN 152, pp. 26, 27, 29; *Rivers in the Desert*, p. 220.

piles of small stones symmetrically arranged[8] in accordance with the contours of the slopes below, and which show also how major and minor channels lead the water collected from both sources in the rainy season down to boxed sections of terraced fields in the wadi below. Outstanding were the major channels which drew off rainwater from the wall-enclosed hilltop of Site 367 to merge with the channel system which laced the rows of small stones and the rows of small piles of stones of the *teleilât el-'anab* into an interrelated whole, and conducted their combined yield to the terraced fields alongside and into the terraced part of the wadi-bed at the bottom of the slopes.[9]

[8] BULLETIN 152, pp. 27, 29, Fig. 4-6; 149, p. 9, Fig. 1 and p. 11, Fig. 2.

[9] I find little reason to change my conclusions with regard to the main function of the *teleilât el-'anab* as a result of Philip Mayerson's interesting article dealing with them in BULLETIN 153, pp. 19-31. The reasonableness of his point, that some of the *teleilât el-'anab* were created by digging holes into the ground in which vines and sometimes trees were planted, with the cleared stones and gravel piled up beside them, is a welcome addition to the general discussion of the subject (cf. BULLETIN 153, pp. 27 and 28, Fig. 6). This may possibly apply to the three lines of *teleilât el-'anab* running parallel to the outer edge of the terraced soil-beds of the Wâdī Isderiyeh and which were well within the lateral wall on the slope above the wadi-bed, as Mayerson suggests (BULLETIN 153, p. 26 and p. 25, Fig. 5). To be sure, I should want to examine the area to determine the relationship of these particular rows to others in the vicinity before being able to accept his conclusion with regard to them completely. Furthermore, there is no question but that Mayerson is correct in asserting that the "flower pot" heaps are in a category by themselves, and cannot possibly be correlated with our belief and that of others with regard to the water-collecting purpose of the *teleilât el-'anab*, but then I do not know that any one has previously claimed such a role for the so-called "flower pot" heaps. It is impossible, however, to accept Mayerson's explanation for more than a fraction of the *teleilât el-'anab*. Nothing he has said seems to me to disprove the evidence adduced to show the connection of the gravel rows ("ridge" rows) and rows of *teleilât el-'anab* with the total water collecting scheme in which, for instance, the wall enclosed hilltop of site 367 and the terraced wadi-bed below it are tied together into one carefully integrated whole. Nor can the almost continuous gravel or "ridge" rows, for instance, which mark parts of the slopes of Qetsiot near Nitsanah, and among which are some sections of *teleilât el-'anab* be explained away as having been thrown up when vines were dug into the ground beside them in the *hammada*. Mayerson's belief that hundreds and thousands of grapevines by the side of such rows could have been irrigated by hand simply will not hold water (BULLETIN 153, p. 27). While Mayerson recognizes that "it was also possible to run small channels on an oblique line from pit to pit, or from trench to trench, which would catch a portion of the slope run-off during the rainy season and direct it into the basins" (BULLETIN 153, p. 27), he does not examine whether or not the soil would retain the moisture under such circumstances.

Mayerson places value upon the tradition still current among the Bedouins in the Negev that these rows of stone heaps were used for planting vines (BULLETIN 153, pp. 26 ff.) He, himself, however, most properly points out that the Bedouins themselves never plant their vines on the *hammada*-covered slopes but in wadi-bottoms. I have repeatedly come across small vineyards and orchards in the midst of the blazing wilderness of the Negev, but they are invariably located in a wadi-bed or its equivalent, behind a dirt wall which dams up the wadi and forces some of the winter and spring rains to sink into its bed and remain in suspension there in the soil (cf. *Rivers in the Desert*, p. 90; BULLETIN 142, p. 33).

There is no question but that much of the region around places like Isbeita, Abda, Ruheiba, and Nitsanah was intensively cultivated in Nabataean-Byzantine

The character of another walled, flat-topped hill, Site 346 (Fig. 2) is also more clearly recognizable from the air than through examinatio on the ground. The wall enclosing this more or less rectangularly shape hilltop is much more massive than that around the top of the hill o

FIG. 2. Wall-encircled site 346.
(Photo Israeli Airforce)

Site 367, but still not high or strong enough to prevent encroachment by man or beast. We have suggested, therefore, that it served to demarcate a sacred area at least as far back as Iron Age II and probably earlier, but that in addition the wall-enclosed area of Site 346 was utilized for water conservation purposes no later than the Nabataean period. Our discovery of *teleilât el-'anab* there reenforced this latter conclusion.[10]

times and heavily planted to vines and fruit trees, and that some of them were on cultivated slopes, although most of them, we are convinced, were located in the exceedingly numerous and intensively terraced wadi-beds. The existence of grape and olive presses and vats in the wadis in their vicinity would alone be sufficient testimony of their existence. We came across formidable evidence of such presses and vats carved out of solid blocks of stone in the Wâdī Sa'adûn opposite Khirbet Sa'adûn (401) in the Negev.

The question as to whether the complex rows of *teleilât el-'anab* and the related gravel or "ridge" rows located almost always on *hammada* slopes materially contribute to collecting run-off rainwater and leading it to cisterns and to terraced wadi-beds is one, the answer to which will be forthcoming from measurements now being undertaken by competent authorities of the Hebrew University. I am convinced that the answer will be positive.

[10] BULLETIN 152, pp. 21-25; 145, pp. 19-20; 149, pp. 9-15.

The airview of Site 346 substantiated both of these conclusions.[11] Two channels, the date of whose construction is uncertain, can clearly be made out from the air leading rainwater in season down the less steep slope of the hill to terraced fields alongside and in the wadi-bed below. It is obvious also that some water descends the opposite steeper slope, to terraced fields of a tiny wadi, which eventually, via a larger, terraced branch wadi, reaches the Wâdī Ḥafîr.

Believing, as we do, that the art of terracing wadi-boxes was extensively practiced at least as early as the time of the Judaean kingdom between the tenth and sixth centuries B. C.,[12] and that indeed some of the techniques of terracing were practiced as far back as the dolmenic period in the sixth millennium B. C.,[13] we are not yet absolutely certain about the time of the initial introduction of the *teleilât el-'anab* and the related gravel strips or rows. That they were extensively employed in the Nabataean period and throughout the Byzantine period for water collecting is now abundantly clear.[14] It may well be that the Nabataeans, who were among the greatest water engineers in the history of ancient civilization, were the first to introduce and make widespread and intensive use of them. It is, however, conceivable and even likely that the Judaeans of Iron II, to whom many of the terraces in the Negev wadis must be attributed and who were also expert practitioners of the art of water conservation, although not in the same class with their successors in the Nabataean and Byzantine periods, may have begun to employ the device of the *teleilât el-'anab* and of gravel rows. The fact remains that although we can pinpoint in the Negev the existence of Judaean terraces and cisterns, of Judaean fortresses and agricultural villages and mining camps, and apparently of several sacred sites—the history of the latter of which can be dated back to the times of Abraham in the Middle Bronze I period,[15] if not earlier—we cannot thus far trace the use of *teleilât el-'anab* and related gravel rows earlier than the Nabataean period.

Each of the two enigmatic, different sized stone circles, near both ends of the long, wall-enclosed Site 346, were again seen, this time from the air, to enclose a comparatively tiny stone circle at its very center.[16] Narrow, wedge-shaped pathways of sorts lead from the outer walls of these circles to openings in the outer wall,—with the narrow point penetrating the outer wall of each circle and the wider end passing through the outer wall on the side facing the Wâdī Ḥafîr below. These pathways are comparatively free from pebbles, and appear to have served as watercourses, conducting rainwater from the two differently sized inner circles, which may at one time possibly have served as auxiliary catchment

[11] BULLETIN 149, p. 13, fig. 3.
[12] BULLETIN 152, pp. 23, 30, 32-34; *Rivers in the Desert*, pp. 202, 215, 217, and fig. 32, 33, 55; Cross and Milik in BULLETIN 142, pp. 5-16.
[13] BULLETIN 152, pp. 24, 30; *Annual* XXV-XXVIII, pp. 189-191; *The River Jordan*, pp. 130, 135, 136.
[14] BULLETIN 152, pp. 25, 27-29; *Rivers in the Desert*, pp. 218-222.
[15] BULLETIN 152, pp. 22, 24.
[16] BULLETIN 145, p. 20.

basins inside the major walled area of the site. It is noteworthy that these two large inner circles are almost completely razed to the ground. It is difficult to establish whether or not they were originally contemporary with the wall proper surrounding the entire hilltop, which may well go back to the Judaean period of Iron II.[17] The flow of rainwater seems to have created another path for itself from the opposite side of the larger of the two inner circles—this one leading down the steeper side of Site 346 in the direction of the terraced branch wadi below, which curves around the base of the hill to join the major Wâdī Ḥafir.

FIG. 3. The so-called "wall" (in foreground) extending towards Borot Lotz from Ras Raman in the Negev.
(Photo Israeli Airforce)

The flight of aerial reconnaissance led us over the top of Râs Ramân and its cairns[18] and over the quite distinct line of the so-called "boundary wall" which leads between it and the originally Iron II cisterns of the Borot Lotz[19] (Fig. 3) and beyond. It could be seen how this strange "wall" was marked at intervals with comparatively small cairns and at terminal points with several larger ones. The "wall" passes along the top or near the top of a rather narrow watershed, following its dips and rises. It looks from the air like a long, undulating serpent pursuing a more or less straight line. Returning a few days

[17] BULLETIN 152, p. 24.
[18] BULLETIN 152, pp. 28, 30.
[19] *Loc. cit.*

later and examining the Râs Ramân and this "wall" again on foot, we found ourselves unable to change our conclusion that whatever else this wall may be, it certainly is not a defensive sort of Chinese wall.[20] It simply could not have served "as a boundary wall to protect the settled Negev population aginst the marauding nomads of the Sinai and 'Aravah deserts," as has been repeatedly maintained by some scholars.[21]

FIG. 4. Remnants of the "wall" leading from Ras Raman toward Borot Lotz in the Negev.
(Photo Nelson Glueck)

A close examination of the so-called "wall" again showed us that it is formed like a necklace of beads, each of which is some distance removed from the one above and the one below it on the single cord on which all of them are threaded. The "beads" consist of a whole series of cairns, each some distance removed from the one above and below, while the connecting "thread" consists of a narrow wall of much smaller width than that of the cairns or "graves" or whatever they may be (Fig. 4). Some of these cairns are more or less circular, while most of

[20] BULLETIN 152, p. 28.
[21] M. Evenari, Y. Aharoni, L. Shanan and N. H. Tadmor, in *The Ancient Desert Agriculture of the Negev, III. Early Beginnings*, in *Israel Exploration Journal*, 8: 4 (1958), p. 250.

them are roughly rectangular, as I have previously pointed out.[22] The drawing of a section of this so-called " wall " which appears in the article by Evenari, Aharoni, Shanan and Tadmor,[23] demonstrates this, and at the same time, in our opinion, demonstrates the impossibility of its being a defensive wall. At best it might possibly be some sort of a property line, in which case the various cairns built on it at irregular distances from one another would merely have accentuated that purpose but never have contributed to its strength as a defense against a marauding enemy. The narrow and comparatively short stretches of wall linking the cairns could easily have been breached in a few hours labor, and simply could not have served as a defensive wall strong enough to stem the advance of marauders. The average width of the narrow connecting sections of the wall is considerably less than a meter and the measurements of the more or less rectangular cairns seem to approximate those we have listed previously.[24] We opened up one of these small cairns and also a section of one of the connecting links of the so-called " wall," and found absolutely nothing. The foundations were only one to three stones deep. No sherds whatsoever were found, although in the previous visit we had found a small number of much worn Iron II fragments by the section of the wall which we marked on our map as Site 140A. If the cairns were graves, as we have thought possible, then they were above ground and have been razed in the course of time practically down to the ground. The purpose of the " wall " remains enigmatic.

The authors of the article in the *Israel Exploration Journal* referred to above have assigned this wall and the cairns connected with it to Middle Bronze I[25] on the basis of what I consider to be a mistaken dating of a small number of potsherds. This is a dangerous method, even assuming that the dating of the sherds were correct. I have found small numbers of definite Iron II and some questionable MB I sherds alongside and in the vicinity of the " wall " and the cairns, but they furnish by themselves no proof positive of the time or times of the " wall's " construction. I have explained elsewhere, for instance, how incorrect it is to establish a date for dolmens by potsherds found in or near them,[26] when it cannot be demonstrated that they are contemporary in origin. Futhermore, the evidence furnished in the photographs of sherds by the above mentioned scholars on Pl. 52 of the *IEJ* article in question does not bear out even indirectly their dating of the so-called " wall " to MB I. Only two of the eight sherds shown, namely the two combed-face pieces of Pl. 52: C can be accepted as being MB I. I am glad to report, that in a letter to me of May 5, 1959, one of the authors, the capable Dr. Yohanan Aharoni, acknowledges that the handmade base built up on a mat shown on Pl. 52 B: 3 of the *IEJ* article is definitely not MB I, but, as he now correctly points out in his letter, is to be assigned to Iron II, being related to one published in his excellent article

[22] BULLETIN 152, p. 30.
[23] *IEJ* 8: 4, 1958, p. 266, Fig. 23.
[24] BULLETIN 152, p. 30.
[25] *IEJ* 8: 4 (1958), p. 249.
[26] *Annual* XXV-XXVIII, pp. 76, 77, 387.

n Rhotenberg's *Taglīyôt Sinai,* p. 137, Fig. 109. He adds in his letter hat the cairn with which this particular sherd is associated " does not belong to the wall but to an Iron Age structure built on the ancient ine." It is difficult to determine from the photograph the age of the herds published as MB I in Pl. 52 A: 1, 2 of the *IEJ* article, but my guess s that they are Iron II and definitely not MB I. The same applies to he three sherds of greyish-black ware with large white grits shown in Pl. 52 E: 7-9, which the authors regard as typical of MB I in the Negev.[27]

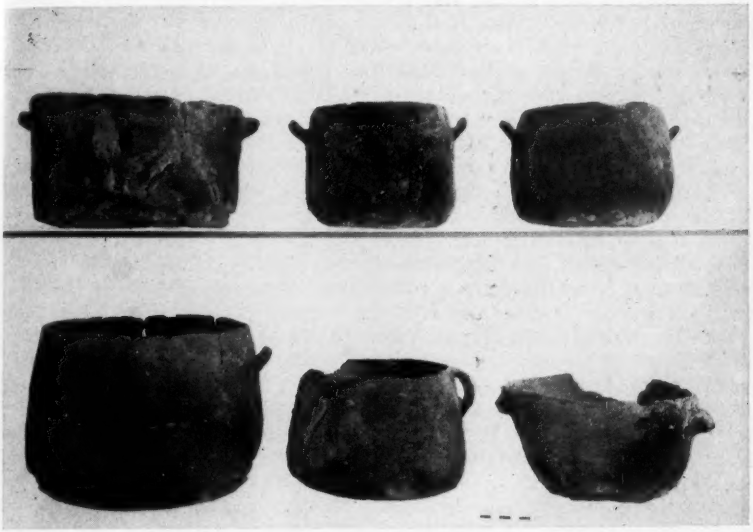

FIG. 5. Iron II, handmade bowls from Tell el-Kheleifeh (Ezion-Geber: Elath). (Photo HUC-JIR)

I am afraid that that is not generally correct, although it applies to some MB I pottery in the Negev. The Middle Bronze I pottery of the Negev is by and large indistinguishable from that of the rest of Palestine or Transjordan, as can be seen, for example, in the MB I sherds from the Negev which we published in an earlier article.[28]

As we have pointed out on numerous occasions, and as the authors of the *IEJ* article show in photographs of Iron II pottery they have found latterly in the Negev,[29] many of the characteristic types of pottery of Iron II in the Negev belong to coarse, hand-made wares. This first became apparent to us from our excavations of Tell el-Kheleifeh (Ezion-geber: Elath) (Fig. 5) and from our earlier discovery of King Solomon's

[27] *IEJ* 8: 4, p. 248.
[28] BULLETIN 137, p. 18, Fig. 4: 1-10, 13, 14; *Rivers in the Desert,* pp. 69, 70.
[29] *IEJ* 8: 4, p. 241 and Pl. 49-52.

copper mines at Mene'îyeh and elsewhere in the Wâdī 'Arabah.[30] A typical, Iron II fragment from Mene'îyeh of a coarse, handmade vessel, built up from a piece of clay anchored on top of a mat, can be seen, for instance, in the sherd collection located in the archaeological building of the Hebrew University. This coarse, largely handmade pottery, which we believe was primarily the handiwork of the semisedentary inhabitants of the Negev in Iron II, is often composed of the same greyish-black ware with large white grits, which the authors of the *IEJ* article submit as evidence in part of the MB I origin of the so-called "wall" and of the cairns of Râs Ramân at the east end of the "wall" and of others like them on or near it farther west. The evidence is as yet insufficient for a definite dating of the "wall" and its associated cairns, despite the existence in numerous places in the Negev of the foundations of roughly circular or oval MB I houses, with courtyards attached to them, which we have discussed in previous articles in the BULLETIN.[31]

It was fascinating to fly over the remains of dozens of ancient sites of various periods, with the megalithic Iron II structures in the Saḥl el-Hawa (Mishor Havah) standing out particularly strikingly, resembling most closely those of the same period we had discovered in Transjordan in earlier years.[32] The flight enabled us mentally to review the history of sedentary or semi-sedentary agricultural occupation of the Negev that we had arrived at after the first three years of work there. We have found no reason to change, but only to enlarge upon our earlier conclusion with regard to the Iron II occupation of the Negev, among others, namely that "the sedentary Iron Age occupation of the Negeb was for the most part limited to Iron II. It was not until the period of great Israelite expansion under Solomon and his successors, when it became possible to exploit such natural resources as the copper mines of the Wâdī el-'Arabah and to expand trade with Egypt and Arabia, aided by the increasing use of water-tight cisterns, that the Negeb south of Beersheba came into its own economically."[33] And agricultural activity kept pace with the rise in military security and commercial traffic.[34]

The water-tight construction of some of the Iron II cisterns has lasted down to the present day, as was demonstrated for us in this flight. Most of the open, more or less circular, dry-stone walled, originally Iron II cisterns of the Saḥl el-Hawa (Mishor Havah) and of the Borot Lotz,[35] were filled with water from the rains of late February and early March, 1959. They sparkled blue behind the whitish marl heaps of debris piled in front of them above the descending slopes below. The more numerous shaft cisterns of the Iron II period could rarely be made out from the air. The channels anciently dug in the slopes above the cisterns of

[30] *Smithsonian Report for 1941*, p. 478; BULLETIN 152, p. 34; 145, p. 23.

[31] BULLETIN 137, p. 15, Fig. 3; 138, p. 15, Fig. 2; 145, p. 16, Fig. 3; cf. *Rivers in the Desert*, Fig. 13, after p. 76.

[32] BULLETIN 152, p. 32, note 40.

[33] BULLETIN 138, p. 12.

[34] BULLETIN 152, pp. 30-36.

[35] BULLETIN 152, pp. 28, 30, 32; *Ma'arîv*, Sept. 14, 1958, pp. 22-23; *Rivers in the Desert*, pp. 173-174, 222.

both types often still function to conduct rain water into them through the original settling basins. These cisterns with their settling basins were repaired and re-used in Nabataean and Byzantine times, and additional ones were dug then. Many of them, together with their settling basins and the channels which led to them, have been repaired and restored in modern times. In some instances, cement walls have replaced the original inner stone block facing, erected not to make these open cisterns watertight (because the interstices between the separate stones are not filled with cement or plaster), but to prevent the marl into which the cisterns were dug from caving in. Evenari, Aharoni, Shanan and Tadmor have explained convincingly that the reason why the stone block linings of these open cisterns were dry built and not cemented or plastered was because the "wet marl itself is impervious to water." [36] Some of these open cisterns and the channels leading down to them have fallen into disrepair, with the result that they are partially or completely silted up. In a number of the Borot Lotz cisterns where this has occurred, trees have rooted themselves and are flourishing.

It was disappointing to see from the air that the reservoir between the two still intact Nabataean-Byzantine dams at Kurnub became so completely silted up during the February-March 1959 rainy season, that it no longer held back any of the freshets that had roared down the Wâdī Kurnub. There were some small pools of water in the wadi-bed below the lowest, easternmost Kurnub dam, but none behind. When we subsequently visited the Kurnub dam on foot, we saw how an unfortunate goat had been swept down by a raging torrent on some earlier occasion, not long before our arrival there, and had been almost completely buried in the silt that was now flush with the top of the easternmost wall. But the modern Israeli Yeruham dam, situated a few miles to the west of it, was brim full.[37]

ISRAELITE SAMARIA AND IRON AGE CHRONOLOGY

G. Ernest Wright

Archaeological discoveries which illustrate the Israelite period in Palestine have multiplied remarkably during the last twenty-five years. Of basic significance chronologically was the stratigraphical and typological fixing of the Philistine period and the age of the United Monarchy by W. F. Albright at Tell Beit Mirsim (Strata B_2 and B_3 respectively) in 1932.[1] The writer's study and dating of Strata III and IIa at Beth-

[36] *IEJ* 8: 4, pp. 241, 242, 260, Fig. 11: 1, 4, 5.

[37] Cf. Fig. 14 and 15, following p. 76 in *Rivers in the Desert.*

[1] *The Excavation of Tell Beit Mirsim*, Vol. I. *The Pottery of the First Three Campaigns* (*Annual* XII, 1932). For architecture, pottery of the fourth campaign, and further comparative treatment see *Vol. III. The Iron Age* (*Annual* XXI-XXII, 1943).

shemesh corroborated and supplemented Albright's results, the second furnishing a particularly valuable addition to tenth-century architecture and ceramic chronology.[2] Then came the first Megiddo volume with its further subdivision of the "Iron Age,"[3] though it was soon apparent that there were stratigraphical and chronological difficulties within it. Albright showed, for example, that in the eastern and southern quarters of the mound of Megiddo the tenth century material had to be resorted and restudied. If this were done, then a new and substantial stratum appeared, mostly from the time of Solomon, one which the excavators had only partially defined as "Stratum IVB." The remainder of IV (IVA) should then be dated between *ca.* 918 and 815 B. C.,[4] when the Aramaeans are to be credited with a destruction of the site. Stratum III is seen to be the eighth century city, destroyed by the Assyrian conqueror, Tiglathpileser III, in 733-32 B. C., while Stratum II represents the Assyrian provincial capital of Galilee before its destruction by Pharaoh Necho in 609 B. C.

With the publication of *Megiddo II* by Gordon Loud in 1948 welcome new material became available, though here again the stratification and the sorting of loci are found to be faulty at almost every point where the results are closely examined. Two phases of Stratum V in the gateway area along the northern side were published, and the writer noted that the latest of these, VA, was to be identified with Stratum IVB in the palace and stable areas in the southern and southeastern quarters.[5] This meant that the 10th century stratum at Megiddo as such was largely missed by the excavators, and now must be reconstructed as "Stratum VA—IVB." To it belong, in all probability, the four-entryway gate, palace and probably one or more of the stable compounds. There may perhaps be an earlier phase, consisting mainly of an administrative center in its own walled compound, which is late Davidic or early Solomonic,[6]

[2] E. Grant and G. E. Wright, *Ain Shems Excavations*, Parts IV and V, 1938-39. Stratum III represented a city under the domination, evidently, of the Philistines; it suffered a violent destruction, after which it was deserted for a time during the second half, presumably, of the eleventh century. Stratum IIa appears to represent a rebuilding in the early tenth century, when this town, like Tell Beit Mirsim (Debir) and Lachish, evidently became a prosperous provincial center of the Davidic government. After the tenth century, however, there was never more than an unfortified village on the site, and no serious attempt was again made to repair or re-use the old Bronze Age fortifications. (See further note 40.)

[3] R. S. Lamon and G. M. Shipton, *Megiddo I. Strata I-IV* (1939).

[4] That is, between the Shishak invasion in the 5th year of Rehoboam and the Aramaean troubles at the end of the ninth century: see *Tell Beit Mirsim III*, pp. 2-3, p. 18 note 6, pp. 29-30 note 10. The Aramaean destruction is inferred from II Kings 13: 3-7 (cf. vss. 22-25 and 12: 17-19) in the reign of Jehoahaz of Israel. The latter is now dated by Albright *ca.* 815-801 B. C. (cf. *BASOR* 100, p. 21). Consequently, the 815 figure for the destruction of IVA at Megiddo should in this chronology be reduced, perhaps even to the last decade of the 9th century.

[5] See the writer's review in *JAOS*, Vol. 70 (1950), pp. 56-60. Cf. also a simplified description of the situation in *BA*, Vol. XIII. No. 2 (May, 1950), pp. 23-46; and *Westminster Historical Atlas to the Bible* (Revised ed., 1956), p. 113. Support for this reinterpretation of 10th cent. Megiddo is given by H. G. May, a member of the Megiddo staff; see, for example, *JBL* LXIII (1944), pp. 191-2.

[6] Whether all or even part of the stable compounds found at Megiddo actually

hile V proper (VB) was evidently a prosperous unwalled town of avidic times in the early tenth century. The two phases of Stratum VI elong to the eleventh century, most of the published material coming om the second half of that century. Stratum VIIA, the last phase of he great Canaanite city, indicated occupation until the last third of the welfth century, when it was violently destroyed as a result of some nknown attack.[7] After that catastrophe, the site remained unoccupied, robably for the better part of a half century, before the comparatively oor village of VI was erected.

These are the strata on which, with Tell Beit Mirsim A, the present ron Age typological chronology of Palestine has been erected and by hich other deposits have been utilized and dated (see Chart). It is o be noted, however, that with the exception of Megiddo IV (IVA) nd III nothing has been said of Palestinian stratification in the ninth nd eighth centuries B. C. The fragmentary information that we had before the last War was summarized by Albright and the writer in dealing with the Tell Beit Mirsim and Beth-shemesh materials.[8] Since, then, he preliminary publications of the discoveries at Tell Qasîleh by Mazar (Maisler) and at Tell el-Fâr'ah by de Vaux give promise of providing important new material for the typological analysis of the period.[9] Of particular significance in this regard, however, has been the publication within the past two years of the details of the Samaria stratification, as recovered by the joint expedition headed by J. W. Crowfoot, ably assisted by K. Kenyon, E. Sukenik and others, between 1931 and 1935,

belong to the 10th century stratum VA-IVB or solely to IV (IVA) of the 9th century is difficult to prove. The same is true of the palace. Below the stables were earlier deposits which had been disturbed by the builders so that insofar as loci are published they give no certain answer. All could be 10th century in origin, and the reference in I Kings 9: 15 (cf. vs 19 and 10: 26) to Megiddo as one of Solomon's chariot cities lends support to the supposition that part of the structures, at least, should go back to his time. On the other hand, the "IVB" palace in its own compound is certainly earlier than the city wall and southern stable compound. Hence it would be simplest to argue that this palace is Solomonic, while city wall, southern stables, and the large palace were IV (IVA). The difficulty here, however, is that the "IVB" palace (with the neighboring buildings IA, 10 and 51) could just as easily be late Davidic or early Solomonic, while the other major structures represent late Solomonic building without major destruction of the site (see also on this point H. G. May, *loc. cit.*). There would appear to be reasonable grounds for believing that in the stable courtyards, and among the foundations, 9th cent. sherds were found (see K. Kenyon in *Samaria III. The Objects*, pp. 199-204). Yet this is no certain or necessary proof of a 9th century construction (as against re-use), especially when the unreliable nature of Megiddo loci is recalled.

[7] Philistine pottery of excellent quality occurs in VIIA, but not in VI, according to this writer's observations from the Megiddo sherd collections in the Oriental Institute. For a detailed study of this stratification see a forthcoming monograph of Gus W. Van Beek.

[8] See W. F. Albright, *TBM III*, pp. 39 ff.; and Wright, *Ain Shems Excavations*, Part V, pp. 134 ff.

[9] See B. Maisler (Mazar), *Israel Exploration Journal* 1 (1950-51), pp. 61-76, 125-140, 194-218; and for Tell el-Fâr'ah see particularly de Vaux's report of the fifth campaign in 1954, *RB* LXII (1955), pp. 575-89.

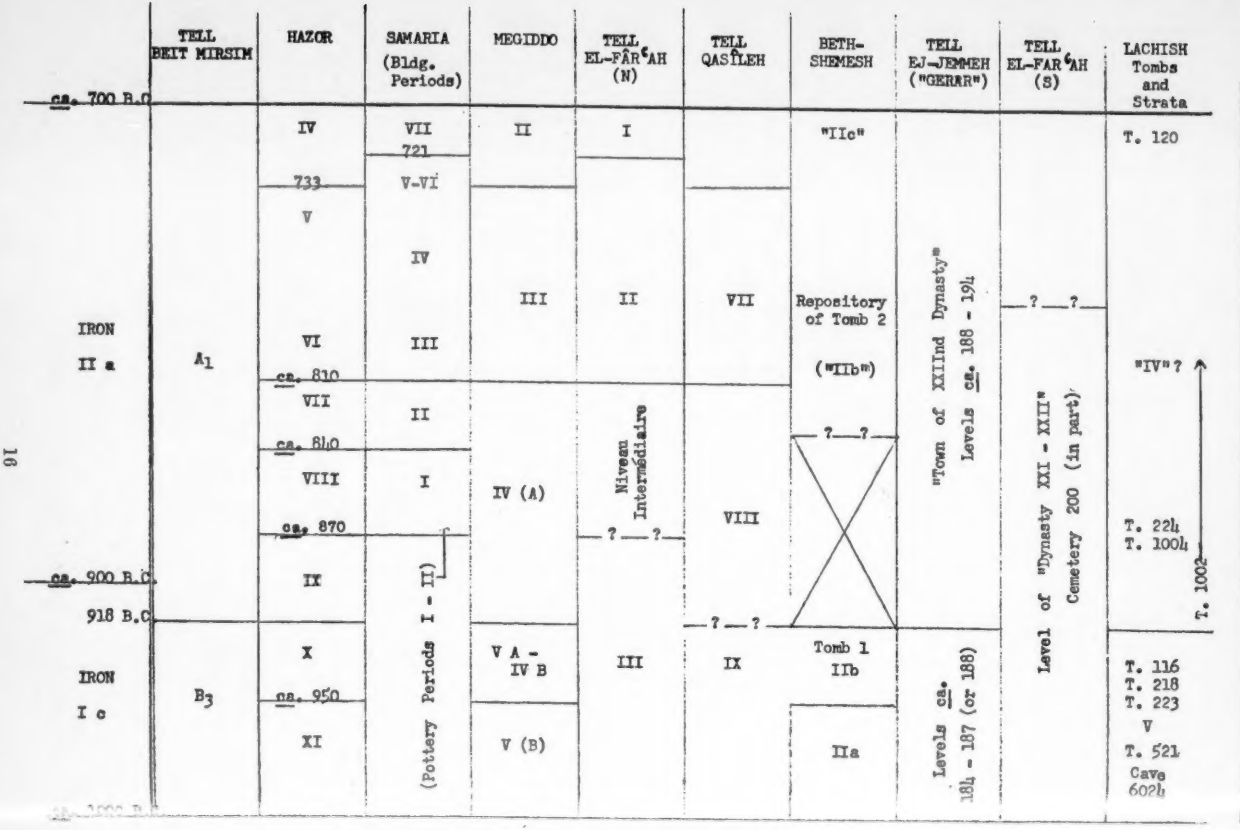

Chart of Related Deposits in Palestine, between ca. 1000 and 700 B.C.

nd the details of the first season of work in 1955 at Hazor by the othschild Expedition of the Hebrew University, directed by Yigael adin.[10] Hazor has six strata between the tenth century and 733/2 B. C., hich have furnished such a wealth of material so closely dated that his site will surely be the future " yardstick " for the archaeology of the inth and eighth centuries. The stratification is as follows:

Stratum X—Solomonic district center of the Province of Naphtali, ending with the Shishak invasion, *ca.* 950-918 B. C.

IX—Early Divided Monarchy before the Omri Dynasty, *ca.* 918-875 B. C.

VIII—Provincial capital of the Dynasty of Omri *ca.* 875-840 B. C.

VII—Reconstruction and repair by the Jehu Dynasty, destroyed by Aramaeans, *ca.* 840-810 B. C.

VI—Rebuilding, partially destroyed by earthquake (cf. Amos 1: 1), *ca.* 810-750 B. C.

V—City completely destroyed by Tiglath-pileser III, and never rebuilt, *ca.* 750-733 B. C.

Samaria as the capital of the Northern Kingdom, with its remarkable city fortifications, palace, ivories and ostraca, is of special interest and importance. Kathleen Kenyon was responsible for a north-south section across the entire summit of the mound a short distance to the east of the palace of Omri and Ahab uncovered by the Harvard Expedition (1908-10). In this area, she has distinguished five building and occupational phases or " periods " between the time of Omri (*ca.* 875 B. C.) and Sargon II's destruction of the site in 721 B. C. A sixth " period " is added mainly from a locus beside a wall (No. 573) which may have served as a retaining (or fortification?) wall for the Israelite road that led up to the city gate along the northern slope.

It must be recalled that Samaria was a major city of Palestine during the later Hellenistic, Roman and even Byzantine periods. Architects of those ages built so well that the stratification below their structures was generally destroyed. The partial remains of the Israelite palace were discovered in the " basement " or rather within the foundations of the Temple of Augustus on the western summit. Consequently, the disentangling of the stratigraphy of the site has been a most difficult task. Miss Kenyon has succeeded only because of her special technique in mapping debris-sections, her detailed debris-analysis, tip lines or " floor " levels in relation to walls, etc. For my part, I have no hesitation in affirming that the Kenyon work at Samaria is one of the most remarkable achievements in the history of Palestinian excavation. The publication of *Samaria III*, when considered together with the extraordinary work done at Jericho between 1952 and 1958,[11] makes her one of the leading field archaeologists of our generation. What follows here is by no means in conflict with this judgment. It is simply an enquiry as to whether, in the light of present-day comparative archaeology, a some-

[10] J. W. Crowfoot, G. M. Crowfoot and Kathleen M. Kenyon, *Samaria-Sabaste III. The Objects* (London, 1957); Yigael Yadin *et al.*, *Hazor I. An Account of the First Season of Excavations, 1955* (Jerusalem, Israel, 1958).

[11] See now Kathleen Kenyon, *Digging Up Jericho* (London and New York, 1957).

Chart of Related Deposits in Palestine, between ca. 1000 and 700 B.C.

what different interpretation is possible of the evidence that has been recovered from Samaria. It must be remembered that the manuscript for both the present volume and for *Samaria-Sebaste I. The Building* (London, 1942) was completed within a comparatively short time after the end of the excavation. The long delay in publication is not the fault of the authors, and it is clear that the manuscript could not be given the extensive revision that its authors could have wished.

The loci from which the stratigraphical evidence was derived were nearly all in three areas. The first group was in relation to the Israelite enclosure walls on the northern edge of the summit. The second group were mainly rooms from a building complex immediately to the south of the fortifications. The third area was a paved courtyard of considerable size, south of the above-mentioned rooms, and interpreted as the courtyard of the Israelite palace which the Harvard Expedition had uncovered immediately to the west.

The earliest occupation is represented by pottery of Early Bronze I preserved particularly in certain pockets in the rock surface. Next, a few sherds and possibly two walls in the building complex, "roughly built, and sealed by the floors belonging to the first coherent plan," are thought to be Iron I.[12] Inasmuch as Omri purchased the site as crown property for the capital of Israel, Period I is credited to him. At this time a remarkably built enclosure wall around the summit was erected of carefully drafted stones, fitted together by the header-stretcher method. It was 1.60 m. wide and enclosed an area *ca.* 178 m. by 89 m. Inside the enclosure a large building was erected along the northern side (the rooms mentioned above); the Harvard Expedition's palace was built at the western side, while a large courtyard existed along the southern side. At least two other walls existed outside the enclosure wall at lower levels.

Period II sees the summit heavily fortified by a casemate wall (two parallel walls with cross-walls in between). On the north side the inner wall was 1 m. thick, the outer one 1.80 m. thick, and the space between 7 m. This fortification was erected outside the Period I enclosure wall, at a slightly lower level, and added some 16.5 m. to the width of the summit platform. On the south side, the casemate construction was much narrower along the western end, while the center and eastern sections appear to have used the Period I wall and to have doubled its defensive strength. No other significant architecture could be attributed to this period. The date for this defensive work is considered to be the reign of Ahab, in agreement with the view of Reisner and Fisher of the earlier Harvard Expedition.

In Period III a wholesale rebuilding of the structure adjacent to the northern enclosure walls, and also of the royal palace to the west, suggests that a catastrophe had brought Period II to a close. The Period I enclosure wall (161) on the north had been abolished, largely removed and built over by an extension of the building being reconstructed to the south of it. The stones employed for this purpose were re-used from

[12] *Samaria I*, p. 94.

earlier buildings; some still had plaster adhering to them. The date for this period is given as *ca.* 840-800 B. C.

Period IV represents no major new construction, but only alterations in the courtyard building, and possibly a stretch of repair in the casemate wall.[13] It is dated *ca.* 800-750 B. C.

Period V is a reconstruction of the courtyard building. Over its floors was "a thick layer of debris, with much burnt matter, including a considerable quantity of burnt ivory."[14] This is probably the evidence for Sargon II's destruction of Samaria in 721 B. C. Period VI is represented by the debris filled up against Wall 573, which may have formed a line for an approach road down the northern slope. There was no stratigraphical relationship between V and VI. Hence VI in Professor Kenyon's opinion could be contemporary with V or it may have been immediately subsequent to it.

The dating of these building phases is derived from an analysis of the pottery, on the understandable assumption that the earliest extensive deposits would belong with Period I from the time of Omri. Futhermore, it is observed from the pottery credited to the various building phases that Periods I and II are very much alike; no real distinction can be observed between them. It is primarily for this reason that the first enclosure wall and the great casemate construction are assumed to have been erected within a comparatively short time of each other, the first in the reign of Omri and the second in that of Ahab. This is admittedly somewhat awkward, however, since Omri could at most have had scarcely half a dozen years at the site. Considering the magnitude of the Period I operation, one would have thought that he could barely have laid his plans, imported architects and materials from Phoenicia, and begun his work, before his death.[15] In any event, the pottery attributed to Period III is seen to be sufficiently different from I-II as to suggest a sizable interval of separation from these strata. A similar interval is assumed as necessary to explain the difference between III and IV ceramically, while IV is very closely related to V-VI.

After a careful and informative analysis of the Megiddo stratification, Miss Kenyon concludes that Megiddo IV and Samaria III are so closely related ceramically that they cannot be far separated from one another, while Megiddo III must be close to Samaria VI. This results in a lowering of the dates of the Megiddo stratification,[16] and the ceramic argu-

[13] This repair could also be interpreted as belonging to Period III (see below). The casement fortification as a whole continued in use until the 2nd century B. C., when it was replaced by the "Greek Fort Wall." It was strengthened in the early Hellenistic period by the great round towers.

[14] *Samaria III*, p. 97.

[15] The figure "twelve" for the length of Omri's reign, including the years of his rivalry for the throne with Tibni (I Kings 16: 21 ff.), is problematical. If Omri began to reign "in the thirty-first year of Asa king of Judah," reigning six years in Tirzah (vs. 23), while Ahab began his reign in the thirty-eighth year of Asa (vs. 29), then Omri could scarcely have reigned more than eight years (on an antedating system). This would leave only *two years* for his residence in Samaria (see Albright, BULLETIN 100, pp. 20-21 n. 15).

[16] *Op. cit.*, pp. 198-204. Megiddo V is dated by Miss Kenyon 1000-850 B. C.;

ments then bolster the contentions of Mr. Crowfoot concerning th architecture, namely, that since the finely dressed and fitted masonry o Megiddo IV is so similar to that at Samaria, the two ought to be con sidered as innovations of the same royal court and of the same archi tects.[17] Since this argument does not take our analysis of the tenth century VA-IVB stratum at Megiddo into consideration, we cannot utilize it except for the general synchronisms of Megiddo IV (IVA) and III with Samaria Pottery III and V-VI respectively.

The arguments against Professor Kenyon's interpretation of the Samaria evidence are as follows:

1. As noted above, Building Periods I and II, the destruction of the major buildings, and their reconstruction (Building Period III), are all dated in one thirty-five year period, between *ca.* 875 and 840 B. C. Not only is it difficult to see how Omri could have completed Period I in his short reign (see note 15), but the interval between II and III is insufficient to permit the development of the differences in ceramic typology which separate Periods I-II from III.[18]

2. When one examines the pottery attributed to Periods I and II according to the chronology reviewed at the beginning of this paper, his immediate conclusion is that this is the type of thing that we have hitherto been dating to the 10th century B. C. That observation has been made also by W. F. Albright and Lawrence A. Sinclair,[19] as well as by two members of the Hazor Expedition, Ruth Amiran and Y. Aharoni.[20] The last two are surely correct, therefore, when they forthrightly state that Pottery Periods I-II[21] at Samaria prove that a settlement existed there in the 10th or early 9th century, "but was completely razed by the great building operations of Omri and Ahab." The evidence certainly suggests that Omri purchased, not a bare hill, but a hill with a village on it, one which Mazar suggests was the estate of an Issachar family.[22]

The question arises, however, as to whether this means that Building

IV, 850-750 B. C.; III, 750-650 B. C. J. W. Crowfoot argues for roughly the same dates, except that of Megiddo IV, which in 1940 he placed in 870-840 B. C. (see n. 17).

[17] *Samaria I*, pp. 7-8; *PEQ* 1940, pp. 132-47.

[18] In the Kenyon interpretation the Period III pottery dates the time of the *beginning* of the phase, not its end, as would normally be the case in other excavations (see below).

[19] See Albright's review of *Samaria III. The Objects* in BULLETIN 150 (April, 1958), pp. 21-3. There he mentions a much worn collared-rim store-jar not later than the 11th cent. and assumes the existence of a hamlet there before the time of Omri, called *Shômerôn*. He dates Period III *ca.* 840-810 B. C. and Period IV, *ca.* 810-745 B. C. Sinclair's observations on the Samaria pottery are published in his treatment of the Gibeah pottery in a volume of the ANNUAL now in press.

[20] See their joint article, "A New Scheme for the Sub-Division of the Iron Age in Palestine," *Israel Exploration Journal*, Vol. 8, No. 3 (1958), pp. 171-84.

[21] This is the writer's suggested terminology for the Samaria pottery stratification to distinguish it from the "Building Periods." It is the contention of this article that the two are not necessarily to be identified.

[22] *Ibid.*, p. 179 note 35.

Periods I-II (the first Enclosure and the Casemate Walls), which Miss Kenyon associates with the pottery in question, also belong to the 10th or early 9th century. The answer may lie in the peculiar situation at Samaria which led Miss Kenyon to try to date building levels by pottery lying *under* the floors or ancient surface levels associated with the walls. This attempt, while necessary, has led the excavator to a very problematical assumption which she explains as follows:

> "At Samaria, the filling overlying any one floor is almost invariably the make-up imported for the floor above. The succession common in a brick-built town, of an occupation layer overlying the floors, succeeded by a destruction level above which the new floors are laid, is not found, since stone walls do not produce continual deposits in the way that mud brick ones do. It is therefore only the pottery of the period of construction that can safely be associated with a building, and not that of the succeeding period of occupation. All the stratified pottery comes from beneath the floors associated with the various walls." [23]

In this view, then, the pottery under a floor or ancient surface level dates the time when the floor or surface was first prepared, as also the walls with which it was associated. This may be true for certain of the latest elements under a floor; but the debris there may also contain much earlier material, and on *a priori* grounds one can have little confidence in an assumption like that used for the Samaria stratification when it becomes a principle for archaeological work in stone houses. A much safer principle would appear to be the one commonly used by other archaeologists: namely that, barring evident disturbance, the material *lying on* a floor comes from the time when that floor was last used, except that above it later artifacts *may* appear in fill used for the foundation of a new floor. If the interval between the disuse of a given floor and the construction of a new one is not great, then the latest pottery *below* a given floor may indeed indicate the approximate time when the floor was laid. Yet whenever fill is present, one can never be certain that the pottery in it is homogeneous. And if the ceramic chronology of a given period is not too clear, then one cannot use fill from *below* floors to establish chronology, unless from other sites he can prove both homogeneity and date. Most of the pottery in fill below floors would be expected to come from earlier occupation, and only a comparatively small amount of it would belong to the builders. In the case of Samaria, then, Pottery Periods I-II must be dated from other deposits before it can be assumed without question that they belong to Omri and Ahab and that other deposits must be redated by them. Until other evidence is forthcoming to the contrary, this pottery is most easily dated in the tenth century, or early ninth century at the latest,[24]

[23] *Samaria III*, p. 90.

[24] See *Samaria III*, pp. 94, 98-107 with Figs. 1-3. No distinction is observed to differentiate Pottery Periods I and II. Note the following characteristics: red-slipped bowls, hand-burnished inside and out (no completely wheel-burnished bowls), types of bowls and craters, the "pilgrim" flask with concentric circles (Fig. 1: 6; Philistine period?), pinch-mouth dipper-juglet (Fig. 1: 7) and an early form of another with round mouth (Fig. 1: 8), 11th-10th century type of jar rims (Figs. 1: 17-18; Fig. 3: 31-33), cooking pot rims with straight or concave exterior rim-

even though architecturally no more than two wall fragments were found which are earlier than Building Period I (see above).

The loci of I Pottery are: on the rock surface between the enclosure and casemate walls, directly south of the enclosure wall, below the floors of the building in this area, and in the "make-up" below the surface of the southern courtyard. In no case, as far as one can see from the plans, is there proof of anything but that this is the occupational debris into which the stones of Building Period I were set.

Pottery Period II is derived entirely from fill imported to raise the ground level between wall 161 (Building Period I enclosure wall) and the inner casemate wall, and the fill thrown into the casemates themselves. All that one can say about such fill, then, is that it would be expected to contain pottery from all earlier occupation levels on the site, the bulk of it being from the period that left the deepest deposit, unless by some accident of digging the builders used only one specific type of debris. While in the last analysis only the excavator knows all the facts needed for final decision, this writer concludes from the published report only that *the stratified debris of Pottery I and II says no more about Building Periods I and II than that the latter are later than the former.* Some sherds may indeed be contemporary, but these are admittedly indeterminate.

3. Of basic importance at this point are the views of Y. Aharoni and Ruth Amiran, field supervisors of the Hazor expedition who were responsible for the areas (A and B) where the Iron Age strata were excavated. Not only have they expressed views similar to those above, but they also have made a reasonably firm correlation between Samaria Pottery III and Hazor VIII.[25] Of special interest is a particular type of

flange (no clear examples of what the Hazor excavators call the "Late Shallow Type": a smaller bowl with sharply grooved rim—Fig. 3: 25, and possibly 26, are the only specimens that are very close to this type). (See further the remarks of Aharoni and Amiran, pp. 178-9.) Miss Kenyon is correct, however, in her statement that as yet we do not have an entirely clear picture as to precisely when such specifically Iron I characteristics give way to those of Iron II. Hence examples of this pottery may indeed come down to the period of the Omri Dynasty. Yet the collection as a whole would then not be homogeneous, because certain elements (e.g. Fig. 1: 6) are surely not later than 11th-10th century. Indeed, if an Iron I village were assumed to have existed at Samaria, then pottery from the 10th and first half of the 9th century would be mixed in the earliest debris on the mound surface. Sinclair in his comparative study of the pottery of I and II reached the following conclusions: In areas where the stratification was not clear, as in the Period I courtyard and the Period II fill between the walls within the casemates, there is considerable mixture of pottery ranging between the 11th and 9th cents. In the building levels south of the enclosure walls the pottery (I) appears more homogeneous and could be dated to the first half of the 9th cent. These conclusions, if time should prove them correct, would lend further support to the arguments here presented.

[25] Since Miss Kenyon had already dated the building of Samaria Period III to *ca.* 840 B.C., this correlation would not be out of line with her chronology. It was pointed out above, however, that on *a priori* grounds the completion of the extensive and beautifully constructed architecture in Building Periods I-II, destruction of the I-II buildings on the summit and their rebuilding (III), all within thirty-five years, is a most unlikely supposition.

b wl which is a prominent characteristic of these two strata. The Hazor a haeologists in their work have called it 'Samaria" Ware. After s ing the type at Hazor in September, 1956, I was enabled to examine t drawers of Samaria pottery in the London Institute of Archaeology a ew days later, and I can vouch for the correctness of the identification. P ofessor Kenyon has given a good description of this pottery as follows:

> "The burnished ware . . . is distinct. The ware is considerably coarser and dirtier than that of Period IV on, but it more closely resembles that than the hard reddish brown ware, often badly fired, of I and II. It appears, in fact, to be of the same clay as IV, but less well levigated and prepared. The slip over which the burnishing takes place is nearer the brownish red of the earlier ware, than the bright red of the latter. The technique of burnishing is interesting. Some hand-burnishing still occurs, but the majority of the bowls are wheel-burnished in a very individual technique . . . Inside, they are so continuously burnished that in some cases it is impossible to see any burnishing lines or to establish the technique . . . ; it is probable that all are wheel-burnished . . . On the exterior, the burnishing is also very good . . ." [26]

What has hitherto commonly been called "Samaria Ware" is a different type of pottery. Its shapes are more elaborate, and Mrs. Crowfoot describes it as being in predominantly buff ware with red slip usually over the whole vessel, except perhaps under the base. Sometimes the burnishing by wheel can be noted on the outside, but usually the vessel has been so polished by hand that the burnishing lines have been eradicated, and the surface given a gloss similar to that on Mycenaean ware or Terra Sigillata. This ware appears in Pottery Periods IV-VI. It is extremely well levigated, of a much lighter paste both in color and weight than the earlier ware, while the red slip is in color a brighter red.[27]

We thus have two types of high gloss or lustrous red pottery from Iron II in Northern Palestine. They are closely related, one following the other, but still distinct. I propose that they be distinguished as *Samaria Ware A* and *B*. Samaria Ware A is now stratigraphically fixed in the 9th century by the contemporary strata, Hazor VIII and Samaria Pottery Period III. It is a comparatively hard, heavy ware, dark gray to black at the core and has only a moderately cleaned paste. The highly polished slip is generally dark red to reddish brown. Samaria Ware B appears to be predominantly an 8th century pottery, found in Pottery Periods IV-VI, in Niveau II at Tell el-Fâr'ah near Nablus, in predominantly 8th century fill over the temple at Shechem, probably in Hazor VI-V, as well as elsewhere.[28]

[26] *Samaria III*, p. 95.

[27] *Ibid.*, pp. 147, 154-6.

[28] The two types of ware have not yet been distinguished from one another at Hazor, though we may expect further and important information from this site on the point at issue. From my own observation, I can testify to the identity of Samaria Ware A in Hazor VIII and Samaria Pottery III, and as well to the difference of Ware B from it, as observed at Shechem and elsewhere, but I did not have opportunity to check on the material in the later strata at Hazor. Yet *Hazor I*, Pl. LIV: 6 (with buff bands left in reserve on the bottom) is one especially fine type of Samaria Ware B, with which compare *Samaria III*, Fig. 9: 2 (p. 120) and

The two types of lustrous wares presumably overlap to some degr , but more evidence is needed to suggest the extent of overlap in ti e and place. Neither, as far as we now know, were extensively import i into Judah, where potters remained content, evidently, with the sim e spiral burnishing on bowls without bothering either with the hig r lustrous surface nor with the special effort required for the paste f Ware B. On the other hand, there would appear to be no questi n but that Ware A began before Hazor VIII and Samaria Pottery I . It is listed in undifferentiated X-IX at Hazor,[29] in Tell Abu Hawam I , and by Gus Van Beek in Megiddo VA-IVB.[30] More study is neede , however, before other factors than a high gloss surface can be used o specify precisely what is meant.

The stratified pottery deposits of Period III were as follows: tw of them were fill connected with walls which either cut through Period I filling (see above) or replaced this filling in conjunction with wall erected after the enclosure wall (161, Building Period I) had fallen into disuse. This would indicate that such walls were indeed later than Building Period I and also later than the time when the fill was thrown into place by the builders of Period II (Casemate Wall). Yet this says nothing about the pottery in the fill either below or associated with III walls, except that theoretically most, or even all of it, would presumably be earlier than the erection of the walls. That some of it filled the older trench from which the stones of the enclosure wall (161) of Period I had been robbed, need suggest nothing more than that II, or even I, occupational debris was used to level up, especially since it is suggested by the excavator that the enclosure wall was partially removed sometime *before* Building Period III. In this debris under a III wall (160) and over the original enclosure wall (161) a fragment of burnt ivory was found which is attributed with hesitation to a partial destruction at

Fig. 19: 1, 3, 4, 7 (p. 156). Hazor VIII (cf. Pl. XLVII of *Hazor I*), on the other hand, appears to have nothing of the sort, though differences between Wares A and B have to be noted from personal handling, unless future study can also discern datable and traceable differences in the form of the bowls. Megiddo IV (IVA) and III should be re-examined, but it will have to be done from such materials as are preserved in the Palestine Archaeological Museum and the Oriental Institute. [Samaria Ware B was also found at Bethel in 1934, 1954 and 1957—Ed.]

[29] *Hazor I*, Pl. XLV: 12, 13, 14, 17. One wonders, however, about the criteria here used to designate the ware as "Samarian." Presumably it is the high polish.

[30] See Van Beek, BULLETIN 138 (April 1955), pp. 36-37, and note 7. There is at the moment a debate on the date of Tell Abu Hawam III. Van Beek argues cogently in the above article for a date between *ca.* 1000 and 918 B.C. (the Shishak invasion), which agrees with the views of Albright and the writer given some years ago. Mazar, on the other hand, has argued for a date between *ca.* 980 and 815 B.C. In this he is independently followed by Aharoni and Amiran (*op. cit.*, p. 177), who would end the stratum *ca.* 840 B.C. Kenyon (*Samaria III*, p. 200) confines it to the tenth century, corresponding to Tell Beit Mirsim B_3. In the light of the new evidence, interpreted in the perspective of this article, I would support the arguments of Van Beek. Among the difficulties in the views of Mazar, Aharoni, and Amiran (see further below) is that of scattering the pottery in one building stratum over a period of a century or more, so that TAH III can be said to contain both 10th and 9th century material on the same floor level. Such a problematic supposition is avoided with the adoption of the earlier date.

the end of Building Period I (or perhaps of II). There is no reason why associated fill in the same area should not represent the same building periods.[31]

The two other main stratified loci listed, are beneath floors of Building Period III in the large structure south of the northern enclosure walls. The published pottery of III comes from this area and from the ceramically (but not stratigraphically) related deep pit Z. As was noted above, there was no discernible building phase in this area between the original construction in Period I and its reconstruction and enlargement in III. Hence one is surely entitled to assume that the bulk of the III pottery should be used to date Building Periods I and II. Because of the extremely difficult stratigraphical situation, one would assume, no deposits that definitely distinguish, ceramically, Building Periods I and II are published. A primary reason may be that no II house levels were found.[32]

Period IV pottery was taken mainly from the "make-up" beneath the new floors in the same building as that mentioned above, and from the fill used to raise the level of the southern courtyard. There would appear to be no published reason as to why the bulk of this pottery should not be assumed to date the final phase of Building Period III, for the most part. A repair of the casemate wall, attributed to this period, could as well belong to Building Period III also. Since the IV Pottery appears to date in the 8th century, the Samaria ostraca presumably must be interpreted as belonging to this phase.[32a] Furthermore, Building Period III may now be interpreted as the rebuilding of the city following an Aramaean destruction at the end of the ninth century.

Similarly, the pottery beneath the new V floors in the rooms south of the casement wall can be assumed to belong mostly to the last phase of IV. In type it is so close to VI pottery, and for that matter also to

[31] *Samaria I*, pp. 101-3.

[32] This raises a problem in relation to Hazor. If Samaria Pottery IV dates the end of Building Period III and is approximately coeval with Hazor VI pottery (*ca.* mid-8th century or just before), then Pottery III, which in the above analysis is all that we have to date Building Periods I and II, ought to be roughly coeval with both Hazor VIII and VII. I have listed it so in the table below, but the matter awaits further study. If Samaria was partly destroyed in the Aramaean invasions *ca.* 810 B.C., then Building Phase III is its rebuilding, and the "III" Pottery just below the floors would be most easily dated to the time just before 810 B.C. (which is the same date as the pottery of Hazor VII). Yet fill, if such it is, is dangerous to rely on. It could be earlier. The matter can be settled in the future through ceramic analysis or further digging.

[32a] The precise dates at which Building Period III gave way to IV and IV to V are conjecture. The Hazor expedition dates the end of its Stratum VI *ca.* 760 on the basis of earthquake evidence and the earthquake mentioned in Amos 1:1. Albright would prefer a date around 745 B.C. for the end of both Hazor VI and Samaria Building Period IV; that would be during the Israelite disturbances following the death of Jeroboam II. As indicated above, however, the only real evidence for Building Period IV consists of a new surfacing of floors in the so-called "ivory house" and a new paving of the southern courtyard. This type of thing could happen repeatedly at any time and does not require major disturbance either in politics or in nature to explain it.

IV, that little time interval between Building Phases IV-V need be assumed.

Professor Kenyon's hesitation about the dating of her Period VI has been alluded to. For my part, I would assume that *Period VI as building phase does not exist*, while the pottery attributed to it should be used for an interpretation of Building Period V.[33] Miss Kenyon's grounds for distinguishing Period VI were as follows: (1) the pottery used to date the V rooms came from fill below floors. In a Period IV house, which continued in use into V, there was a rubbish pit, filled with pottery. Miss Kenyon thus says that the pottery below the floors dates the time of the construction, while that in the pit dates the period of the destruction.[34] (2) Down the slope where Wall 573 presumably kept debris from the approach road to the city, there was a considerable raising of level in the dirt related to this construction. The pottery from the area is the same as that in the above-mentioned pit. It thus is a later phase than that below V floors. For this reason it is given a separate designation, "VI." Yet lacking a solid stratigraphical basis to indicate a *building phase* definitely later than V (not simply *in use* later than "V pottery"), it seems to me that the designation should be abandoned. For clarity's sake, however, I shall refer to it as "Building Period V-VI," and assume that the two are actually the same.

The following, therefore, represents this writer's interpretation and dating of the Samaria stratification in relation to that at Hazor, it being understood that any such chart oversimplifies the evidence:

Samaria			Hazor
10th-early 9th Century		Pottery Periods I-II	X-IX
ca. 870-842 B. C.	Building Period I	Pottery Period III	VIII
ca. 842-810 B. C.	Building Period II (?)	(See note 32)	VII (?)
ca. 810-± 750 B. C.	Building Period III	Pottery Period IV	VI
ca. 750-± 735 B. C.	Building Period IV	Pottery Period V	V
ca. 735-721 B. C.	Building Period V-VI	Pottery Period VI	—

The important article of Y. Aharoni and Ruth Amiran ("A New Scheme for the Sub-Division of the Iron Age in Palestine") has been repeatedly referred to in the preceding pages, and a few additional comments must now be made. The main suggestion in this article is that a new tripartite subdivision of the Iron Age be adopted. Instead of the old division into two three-hundred-year periods (Iron I or Early Iron, *ca.* 1200-900 B. C.; Iron II or Middle Iron, *ca.* 900-587 B. C.), the following is thought to represent ceramic realities more adequately: Israelite I, 1200-1000 B. C.; Israelite II, 1000-840 B. C.; Israelite III, 840-587 B. C.

The length of time suggested for "Israelite III" is awkward unless it is understood that it must be subdivided to separate 8th from 7th and early 6th century pottery. The suggestion that we adopt "Canaanite" and "Israelite" instead of "Bronze" and "Iron" is, however, one which this writer has long felt desirable because of the great difficulties involved in using the older terms. Yet the main problem is the desirability of a terminology which will apply to both Syria and Palestine, and for this purpose "Israelite" and, in the Iron Age, "Canaanite" are too geographically confined.

The most important part of the suggestion is that involved in "Israelite II."

[33] This also includes, presumably, most of the other loci (except the Deep Pit Z) treated by Mrs. Crowfoot in *Samaria III*, pp. 134 ff.

[34] *Ibid.*, p. 108.

This writer for some time has also understood that the first main division in the Palestinian Iron Age occurred at the end of the Philistine Period and the beginning of the United Monarchy (*ca.* 1000 B.C.). The period from 1000 to 600 B.C. is one cultural unit—not that tenth and seventh century pottery can be confused, but that the period is tied together in a gradual inner development. Furthermore, it has long been clear that the dividing point between new 10th century ceramic developments, on the one hand, and those of the 8th-7th centuries, on the other, did not occur precisely at the division of the kingdom (*ca.* 922 B.C.), nor even roughly at 900 B.C., but during the course of the ninth century. Such developments would include among the more obvious features the increasing use of wheel-burnishing ("ring-burnishing"), and substantial typological modifications in small juglets and cooking pots. On the basis of the discoveries at Hazor since 1955 the Israeli archaeologists now suggest that this dividing point can be more precisely fixed. That is between Hazor VIII and VII, at the conclusion of the Omri Dynasty, when the revolts of Jehu in Israel and Athaliah in Judah took place, when the conquered territories in Transjordan were lost and Aramaean pressure from the northeast increased.

With these observations in general this writer is prepared to agree. It is only with too rigid an application that one begins to be uncomfortable. For example, Tell Beit Mirsim B_3 is now dated 1000-840 B.C. to agree with the theory. Tell Abu Hawam III is given the same date (see above, note 30). Samaria III (evidently referring to Pottery Period III) is dated *ca.* 875-840 B.C.; IV, *ca.* 840-815 B.C.; V, *ca.* 815-765 B.C.; and VI, *ca.* 765-721 B.C. If there is any merit in the stratigraphical and ceramic analysis given in this article, then it is difficult to defend such dates for Samaria Periods IV and V, nor is it entirely clear that III can be so narrowly confined (see above, and especially note 32).

Be that as it may, the ceramic arguments for the division at 840 B.C. do not as yet appear to this writer as completely settled, though in general the position appears sound. The first is in regard to the cooking pots, a separation between the Early and Late Shallow Types taking place between Hazor VIII and VII. If the pottery of VIII and VII, however, dates from the end of the respective period of each, all one can say is that the transition occurs during the second half of the 9th century.[35] In bowl types a straight-sided flat-based bowl is said to begin in Hazor VII. I would understand this to mean, however, that it only came into fairly common use in the second half of the 9th century.[36] The common water decanter of 7th century Judah is assumed to begin in Hazor VII and to continue with no real changes through Strata VI and V. From the published examples, however, it is by no means clear that the typical Judaean type with the flat, strap-handle has been found at Hazor at all.[37] An analogous form appears at Samaria, beginning in Pottery Period III, but it is hardly the well-known Judaean type.[38]

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[35] Cf., however, *Hazor I*, Pl. XLVIII: 3 which appears to be a Late Shallow Type attributed to Stratum VIII. The situation at Samaria with regard to cooking pots is by no means clear, and one suspects (1) either that the Hazor criterion cannot be too rigidly applied, (2) or that individual sherds may easily be out of place in such an extremely complex stratification as that at Samaria. At the moment, this writer suspects that both may be true, though he has no evidence except hints from 8th century fill at Shechem.

[36] Cf., however, *Hazor I*, Pl. XLIX: 10, for one type of this bowl attributed to Stratum VIII. At Samaria it is first listed in both Pottery Periods II and III (*Samaria III*, Fig. 3: 3; Fig. 4: 13), though it is common only in the 8th century periods.

[37] Cf. *Hazor I*, Pls. L: 23-25; LVI: 18 (?); LXII: 12 (?); LXXII: 7.

[38] Cf. *Samaria III*, Figs. 5: 1; 10: 17; 22: 1-5. The strap handle does not appear at Samaria, nor does the typical Judaean burnishing on the vessel's body. *Megiddo I*, Pl. 4 shows the various forms found at Megiddo, four of which (Nos. 97, 99, 100, 107) are said to appear in Stratum IV (IVA of second half of 9th century). Nos. 99, 100, and 103 on the plate look close to the Judaean shapes, but they appear to

Consequently, firm conclusions from the few specimens appearing in Hazor V cannot be pushed too far, unless we were to say that a type from which the la Judaean form is developed during the second half of the 8th century, first appe in Israel as early as the middle or second half of the 9th century (Samaria Pott Period III and Hazor VII). In other words, that a ceramic shift occurred in 9th century seems clear, but we need more information before it can be defi with precision, especially in view of the regional differences between northern a southern parts of the country.

In conclusion, with regard to the perspective with which the tenth century depos at Tell Beit Mirsim and Beth-shemesh is viewed, it may be noted that our Isra colleagues appear to doubt whether the Shishak invasion of *ca.* 918 B. C. actua destroyed any of the cities of Judah. One basis for this doubt appears to be t supposition that Rehoboam paid tribute to the Egyptian ruler, became his vass and thus avoided trouble. Yet I find this interpretation of the situation difficu to follow. Not only can it be argued that there is room for a list of Judaean citi within the gaps of the Shishak inscription,[39] but indeed if one takes seriously t Chronicler's presentation of the tradition, one must assume that Shishak struc hard at Judah and that Jerusalem itself was only saved by the payment of tribut just as was the case in the campaign of Sennacherib in 701 B. C. (2 Chron. 12: 1-12, with specific reference to the capture of the fortified cities, except Jerusalem, in vs. 4).[40] Consequently, the invasion can still be used to interpret major interruptions

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have no strap handles and no burnish. On p. 163 the excavators say that the form was rare in Strata IV and I, but found in profusion in III (8th century). The hard metallic ware common in Judah is only in III-II (8th-7th century). Here again the forms are not good examples of the best Judaean types. *RB* 62 (1955), p. 577, Fig. 16: 12 is an early example from Tell el-Fâr'ah, Niv. III (10th or early 9th century). Its globular body closely resembles *Samaria III*, Fig. 22: 5, of Pottery Period III.

[39] According to the opinion of Albright, based on the discovery by B. Grdseloff and B. Mazar that the list should be read *boustrophedon* (that is, the lines being read, first, from left to right, then from right to left, then left to right, etc.); cf. BULLETIN 130 (April, 1953), p. 7; and *TBM* III, p. 38. Noth, however, in his *History of Israel* (Edinburgh and New York, 1958; tr. by Stanley Godman), p. 238 (cf. further his article in *ZDPV* 61, 1938, pp. 277-304), and B. Mazar maintain that Judah was not attacked. The conclusions of the last mentioned are clearly and persuasively argued in "The Campaign of Pharaoh Shishak to Palestine," *Volume du Congrès, Strasbourg, 1956* (*Supplements to Vetus Testamentum*, Vol. IV, Leiden, 1957), pp. 57-66. In this analysis of the *route* of Shishak, the latter goes from Gaza along the coastal plain to Gezer, up the Valley of Ajalon to Beth-horon and Gibeon, then to the Jordan Valley at Adamah, across to Gilead, back up the Wâdī Fâr'ah to Tirzah, to the Plain of Jezreel (Beth-shan, Taanach, Megiddo), south to the Plain of Sharon, to the Negeb and Edom. On this basis, Rehoboam saved his country from the serious destruction visited on Israel. Nevertheless, even if this view proves correct, it does not necessarily mean that such Shephelah cities as Tell Beit Mirsim, Lachish, and Beth-shemesh were not attacked (perhaps even before the payment of tribute), nor even that Gibeon may not have been forcibly captured. Professor Mazar believes that Tell Jerîsheh and Tell Qasîleh were destroyed by Shishak, though they are not mentioned in the list. In any view of the Shishak campaign there are so many unknowns that archaeological data are needed before final judgment is reached. Furthermore, the Chronicler's treatment and interpretation of the event cannot be lightly dismissed, even though he is using more than one source of tradition (according to Mazar, a temple chronicle and a prophetic history). See further, on the Chronicler's account of Shishak's raid, W. F. Albright, *JBL* LXXI (1952), p. 250.

[40] A brief word should perhaps be said about Beth-shemesh IIb. In an article on "The Boundary and Province Lists of the Kingdom of Judah," by Frank M. Cross, Jr., and the writer (*JBL* LXXV [1956], pp. 202 ff.), it was suggested that

in he archaeological stratigraphy of the late 10th century throughout the country, un ss the Judaean hill country south of Jerusalem be excepted.

may be noted that, contrary to the Aharoni-Amiran assumption, few archaeol ists in the past would have maintained that the dividing point between Iron I an Iron II is precisely the division of the Solomonic kingdom or the raid of Shishak. C tural developments, especially ceramic changes, generally take time, and it has co monly been assumed that the major ceramic changes between Iron I and II to a long period to develop, especially since what has been described has been fo the most part the shift between the pottery of the 10th-9th century and that of th 8-7th centuries. Nevertheless, I hope that the Aharoni-Amiran suggestions co cerning terminology can be further discussed and refined, particularly as developm nts in typology and technique during the 9th century become increasingly clear.

THE SACRED STONE CIRCLE OF KHOR RORI (DHOFAR)

RAY L. CLEVELAND

During the extended campaign of excavation carried out at the site of the pre-Islamic South-Arabian port city of Sumhuram [1] in 1952 by Dr. Frank P. Albright,[2] an important temple dedicated to the moon-god Sîn was discovered inside the north wall of the city just west of the gate.[3] No other cultic installation in the city or in its immediate vicinity was identified at that time.

However, in February of 1958 while the writer was making further investigations of archaeological sites in the Province of Ḍhofâr,[4] a semi-

there was a gap in the occupation of the site, corresponding to the interval between Tomb 1 (10th century) and the Repository of Tomb 2 (8th century), that is, through part or most of the 9th century. A large collection of Stratum II sherds from the last Grant campaign in 1933 are identical with TBM B_3, while the site also has a great deal of material from the 8th and 7th centuries. Since such a collection of B_3 material would hardly have been preserved without some disturbance which both broke and preserved the pottery, it was stated that the site was probably destroyed by Shishak in 918 B. C., and not reoccupied for some time. In any event, it was not refortified after the 10th century. Aharoni and Amiran object to this interpretation and point to 9th-8th century pottery as proof that IIb is later than the 10th century. In defense may I say that in 1937 I worked on the Beth-shemesh material under extreme difficulty and with very inadequate records. Many important sherd collections, especially from the 1933 campaign, could be stratigraphically fixed from Stratum IIa on down. But most of the material allotted to IIb and IIc was distributed solely by typology; I had few *stratigraphically* fixed loci to depend on. Hence an attribution to IIb usually only means a II sherd later than IIa but surely earlier than the 7th cent. (IIc). These latter were not clearly distinguished stratigraphically.

[1] This is the probable vocalization of *smhrm* of the inscriptions (it also occurs without *h*).

[2] See the last chapter of *Qataban and Sheba* by Wendell Phillips for an account of this expedition; also *Antiquity*, Vol. XXIX, No. 113 (March, 1955), pp. 37-39.

[3] Frank P. Albright, "The Himyaritic Temple at Khor Rory (Dhofar, Oman)," *Orientalia* 22 (1953), pp. 284-287.

[4] Cf. BULLETIN 153, p. 11.

Pre-Islamic Sumhuram on the edge of Khôr Rôri. The rock mass, with the stone circle to the left of it, can be seen at the extreme right. The spring is at the lower center.

(Photo by F. P. Albright, courtesy [illegible])

ircular row of rough stones of fairly large size, located some 250 meters ortheast of the city of Sumhuram itself, attracted attention because f their orderly arrangement and the fact that many of them were still tanding on their sides. The ends of the semicircle ended at a low outropping of rock, with the circumference centering on a somewhat promient rock mass under which a low cave could be discerned. The cavern vas filled with loose soil (apparently blown in by the strong winter vinds) to such a height that entrance was precluded without prior xcavation.

The investigator strongly suspected this to be some sort of ritual nstallation, but his ideas were not crystallized until later. Study of the bagan *ṭawâf* of north and central Arabia in connection with his doctoral lissertation [5] and perusal of Philby's description of a larger stone circle n Nejrân [6] provided the answer. Here on the south-central coast of Arabia is another example of the cultic processional way around a sacred rock mass so prominent in ancient Semitic religion, to be connected indirectly with the *bâmāh* high place of Canaanite cult.[7] Further investigation of the sacred circle of Khôr Rôrī, suggestively located near a perennial spring of drinkable (though distasteful) water, is in order, and detailed photographs, a plan, and a full description can be expected to appear in the full report of the excavation of Sumhuram now being prepared.

DUNAND'S NEW BYBLOS VOLUME: A LYCIAN AT THE BYBLIAN COURT

W. F. Albright

The plate volume of Maurice Dunand's *Fouilles de Byblos, II* (*1933-1938*) was published in 1950, and was followed in 1954 by the first half of the text volume; both were duly noticed in these pages. Now we have the second half of the text volume (Paris, Adrien Maisonneuve, 1958, quarto, pp. 479-1082), and can utilize the rich content of the photographs in the plate volume illustrating the Temple aux obélisques and related sacred areas. For the extraordinary wealth of material dating from the last centuries of the third millennium and the beginning of the second, we owe the author a great debt.

His datings seldom appear to be in substantial need of correction;

[5] *The Excavation of the Conway High Place* (*Petra*) *and Soundings at Khirbet Ader*, to appear as Part II of *Annual* XXXIV-XXXV during the winter of 1959-60. Chapter III, "The High Place in Ancient Semitic Religion," contains the relevant material.

[6] H. St. J. B. Philby, *Arabian Highlands*, pp. 220-224.

[7] See W. F. Albright, "The High Place in Ancient Palestine," *Supplements to Vetus Testamentum*, Vol. IV (1957), pp. 242-258.

they are based chiefly on the rich contents of the deposits of offerings (*dépôts d'offrandes*), rather than on the scanty objects found above the floor of the Temple aux obélisques. There are a few intrusive objects from the temple *levées* (and perhaps one or two attributed to the *dépôts*), which can clearly be dated in Middle and Late Bronze,[1] but there can be no doubt about the overwhelmingly E. B. III character of the deposits in question. Not only are there none of the M. B. II vases and other objects (such as toggle pins), which one would normally expect after ca. 1850 B. C., but there is not a trace of the caliciform ware which dominated Syria and Palestine during the 21st-19th centuries B. C.[2] Art objects are plentiful, though mostly simple and often extremely primitive. Moreover, in striking contrast to later Phoenicia, the art of Byblos at that time was more closely tied to Mesopotamia than to Egypt. There are many parallels to material in the Royal Tombs of Ur (ca. 25th century) and later. If we had any comparable burials or other deposits from the Dynasty of Accad, we should probably find still closer parallels. One crude scarab with an obscure inscription which was presumably not understood by its maker, suggests Sixth Dynasty influence.[3]

The Temple aux obélisques, with its "antichambre," "procella" and "cella" is quite similar in basic plan to the Late-Bronze temple of Hazor, excavated by Yigael Yadin in 1957-58.[4] As correctly emphasized by the latter, the Hazor temple is a precursor of the type now familiar to us from Solomon's Temple and the somewhat later royal shrine of Ta'yīnât in northern Syria. We now see from the plan of the temple in its second phase (*Byblos, II: Texte*, fig. 767 on pp. 640-641) that this type of structure was authentically Phoenician, going back at least to ca. 2000 B. C. So Phoenicia has at last yielded direct evidence for the early evolution of the plan followed in the Temple of Solomon!

[1] See pp. 948-954 for a discussion of chronology, ending with the judicious statement that the deposits were put into their resting places in the Temple aux obélisques early in the Twelfth Dynasty.

[2] For the chronology of the caliciform period in the Orontes Valley see E. Fugmann, *Hama . . . l'architecture des périodes pré-hellénistiques* (1958), pp. 80-85, 281 f. (radiocarbon dates). The date finally reached for all eight phases of Stratum J is ca. 2350-1900. Most of the phases were very thin, and it was naturally often impossible to be sure of the exact attribution of caliciform ware to a given phase. The total thickness was about four metres, but it was mostly débris of mud brick, usually on foundations of field stones, and the rate of accumulation was probably very fast. At Hajar Bin Ḥumeid in Qatabân we found (1950-51) that there was a total accumulation of over fifteen metres in about a thousand years. Since there were probably periods of abandonment there, and since the rainfall was much less in the Wâdī Beiḥân than in the Orontes Valley, the four metres need not have covered more than 250 years—say about 2100-1850 B. C. At all events, we can be reasonably sure that the caliciform period did not begin until after the Empire of Accad (ca. 2350-2200), whose pottery was quite different. The radiocarbon dates are useful, but look a little high to me.

[3] Note, e. g., the repetition of the hieroglyphs *nfr k*^{3} on both sides; *Nfr-k*^{3}-*r*^{c} was the prenomen of Phiops II.

[4] Cf., e. g., the excavator's reports in *Israel Exploration Journal*, IX (1959), and plate 10; *The Biblical Archaeologist*, XXII (1959), pp. 3 ff.

In this review article we shall devote ourselves to the obelisk with an Egyptian inscription in two columns, published by Dunand in the plate volume (*Byblos II*), Pl. XXXII: 2, and described, with transcription and translation, in the text volume, p. 878, on No. 16980. The photograph is not sufficiently clear to be read through without the transcription. The obelisk was hewn of hard limestone and consists of three parts, a square plinth at the bottom, a tapering four-sided shaft, and a pyramidion at the top. It was one of a whole series of "obelisks," most of which were only rough steles.[5] No other example of a true pyramidion was found. The obelisk in question was only 1.25 metres high above the plinth.

The inscription reads in transcription: (1) *mry Ḥr-š·f ḥ3ty-ʿ n Kpny b-šmw wḥm ʿnḫ* (2) . . . *f Kwkwn s3 Rwqq m3ʿ ḫrw*, which we may render:

"Beloved of Arsaphes, Abishemu, prince of Byblos, renewed in life, his . . . , Kukun, son of 'the Lycian' justified (i. e., deceased)."

Dunand is quite right is suspecting that this Abishemu (who bears a name composed of the same elements as *Sumu-abum*, in reverse) is probably not the same person as the Abi-shemu who succeeded Yapa-shemu-abi[6] as prince of Byblos toward the end of the 19th century. Not only do the contents of the *dépôts d'offrandes* suggest a date not later than the 21st century, but the name of the great ram-god of Heracleopolis Magna, divine suzerain of the Heracleopolitan kings of the Ninth and Tenth Dynasties, also suggests a date in the same range. It is possible that this Egyptian god was assimilated to Cananite Resheph, but since the name which became *Arsaphes* in Hellenistic Greek was then pronounced something like *Eḥrišîf*,[7] "He Who Is over His Lake," an identification with *Rašpu* or *Rašap* is not exactly favored by assonance. The absence of recognized Heracleopolitan inscriptions at Byblos is no proof that the influence of these kings was not strong there, since Dunand has long since shown that the majority of the inscribed Egyptian objects found at Byblos, belonged to structures which had been destroyed by fire at the end of the Old Kingdom (22nd century B. C.). It has long been known that the Delta cities became important autonomous centers about this time. It is scarcely necessary to point out that the cult of Arsaphes at Byblos cannot have long outlasted the conquest of Phoenicia by the Theban kings of the Twelfth Dynasty.

The "obelisk" in question was not raised in honor of the living Abishemu (who no doubt rated much more impressive memorials)

[5] On the purpose and Hebrew affinities of these mortuary steles see my discussion of "The High Place in Ancient Palestine" (*Vetus Testamentum, Supplements*, Vol. IV, 1957, pp. 252 f.).

[6] Now known from several occurrences to have been a good Amorite name in the Mari period.

[7] On the vocalization of the first element, *ḥr(y)*, see my discussion in *Journal of Near Eastern Studies*, V (1946), p. 14, No. 16. The second element, *š*, became *šēi* in Coptic and appears in Hebrew transcription as *šî*, in Greek as *si*. Long *i* in a closed, accented syllable became *ĭ* in the second millennium and *ă* after the 13th-12th centuries B. C.

but of the deceased Kukun, son of R(L)uqq. These names are no Egyptian, nor are they Semitic or Hurrian. The orthography is characteristic of the execration texts published by K. Sethe and G. Posener, in which *W* is regularly written for the vowel *u*, and the sign *RW* (the lion) represents *ru*.[8] A date about 2000 B. C. is thus perfectly reasonable from the standpoint of orthography alone. The name of the deceased is well known; it appears in the Hittite documents as name of a king of Wilusha, written *Kukkunnis* (with the Hittite nominative ending). Wilusha was certainly in southwestern Anatolia and has been identified by A. Goetze with Lycia[9]; Kukkunnis flourished in the fourteenth century and was succeeded by Alaksantus. As recognized by J. Friedrich, J. Sundwall, and E. Laroche, *Kuk(k)un(n)i* was an authentically Lycian name; cf. the epichoric *Kukuneye* (*eye* is case ending).[10] Any doubt should be removed by the patronymic *Ru-q-q*, "The Lycian." Gentilics were used in Middle Egyptian without consonantal change as personal names; cf. H. Ranke, *Ägyptische Personennamen*, p. 59: 2 (*ʿꜣmw*, "Asiatic") and p. 227: 29 (*Rtnw* = "Syro-Palestinian"). In the Ugaritic tablets, where *Lky* and *Lkn* (Hittite *Lugg-*, Amarna *Lukk-*) both mean "Lycian," like *Mṣry* and *Mṣrn*, "Egyptian," such gentilics are used over and over as patronymics; e. g., *bn Lky*, "son of the Lycian."[11] It must also be stressed that there is no phonetic difficulty, since *RW* can equally well be read *lu* (initial *r* was apparently not found in Anatolian names), while Egyptian *Q* interchanges with *K* in writing *k* in non-Semitic foreign names. Doubling of a consonant is also well attested, though rare, in Egyptian transcriptions.

It is actually no more surprising to find an Aegean or Anatolian at the court of Abishemu ca. 2000 B. C. than to find Aegeans and Anatolians at Ugarit in the 14th-13th centuries B. C. During the Twelfth Dynasty much Kamares ware was imported into Egypt from Crete, and there are many indications of other cultural relations between north and south.[12] Byblos was much more important, relatively speaking, in Middle Bronze than in Late Bronze.

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[8] See my discussion in *Journal of the American Oriental Society*, 74 (1954), p. 224. My comparisons between the Sethe lists (from between ca. 1925 and 1875) and the Posener lists (from the second half of the 19th century) were based only on the Semitic names; it should be pointed out that there are also two examples of *RW* among the Nubian names in the Sethe lists, though we cannot say anything about their vocalization.

[9] Cf. most recently the map of Asia Minor in the Hittite period published in the second edition of his *Kleinasien* (1957), after p. 228.

[10] See Friedrich, MVAAeG 34: 1 (1930), pp. 88 f.; Sundwall, *Kleinasiatische Nachträge* (1950), p. 27; Laroche, *Recueil d'onomastique hittite* (1952), p. 25.

[11] For Ugaritic gentilics and patronymics based on them see most recently M. Astour, *Revue d'Assyriologie*, LIII (1959), pp. 71 ff.

[12] See especially Helene Kantor, in Robert Ehrich's *Relative Chronologies in Old World Archaeology* (1954), pp. 10 ff. (with which cf. my observations, *ibid.*, pp. 32 f.). Numerous silver vases from the Ṭôd deposit of the time of Amenemmes II (late 20th century B. C.) show a plain or torsional fluting of Minoan origin (from which the decoration of the Kamares ware was in part derived).

SOME RECENT BOOKS (continued)

Great Ages and Ideas of the Jewish People, by **Salo W. Baron, Gerson D. Cohen, S. Halkin, Yehezkel Kaufmann, Ralph Marcus,** edited with Introduction by **Leo W. Schwarz** (New York, Random House, 1956, xxvii + 515 pp.) is an extremely valuable work, containing contributions by noted scholars to an engrossing theme. By far the most important single item is the long chapter by Kaufmann, into which he boils down the essence of his great work on the "History of the Religion of Israel." Until the University of Chicago Press issues its translation of selections from Kaufmann by Dr. Moshe Greenberg of the University of Pennsylvania, this will remain the best introduction for Western scholars who do not read Modern Hebrew with ease.

John Bright's monograph, *Early Israel in Recent History Writing* (*Studies in Biblical Theology*, No. 19, London, SCM Press, 1956, 126 pp.) is an admirable example of critical analysis of recent scholarly work. With unvarying courtesy he discusses the approach of Martin Noth and Yehezkel Kaufmann to the Israelite occupation of Palestine, concluding that both are wanting—Noth's because of its unwillingness to use archaeological evidence and its cavalier treatment of biblical tradition, Kaufmann's because of its refusal to accept any of the modern Western analysis of this tradition. We look forward eagerly to the forthcoming publication of Bright's "History of Israel."

Ernst Ludwig Ehrlich has given us a masterly sketch of the history of Israel in his *Geschichte Israels von den Anfängen bis zur Zerstörung des Tempels* (70 n. Chr.) (Sammlung Göschen, Band 231/231a, Berlin, de Gruyter, 1958, 159 pp.). This is easily the best currently available survey of the subject in this limited size.

In *Von Ugarit nach Qumran* (*Otto Eissfeldt Festschrift*, Berlin, Töpelmann, 1958, pp. xvi + 304) we have a worthy testimonial to a great German scholar, issued in connection with his seventieth birthday. The volume contains twenty-three papers from nearly a dozen countries; some of the contributions are outstanding.

Two important volumes from the pen of **Gerhard von Rad** have recently appeared: *Theologie des Alten Testaments*, Band I: *Die Theologie der geschichtlichen Überlieferungen Israels* (Munich, Chr. Kaiser, 1957, 472 pp.); *Gesammelte Studien zum Alten Testament* (Munich, Kaiser, 1958, 312 pp.). The former is the first Old Testament Theology to be published by a member of the inner circle of the great school founded by **Albrecht Alt,** and is closely analogous in its point of view to **Martin Noth's** historical approach. The reviewer differs quite widely from Alt's pupils, who have followed a course which was implicit in some of their great teacher's work, but tends to go far beyond it. (The reviewer is now two-thirds through the first draft of a "History of the Religion of Israel," which may be described as intermediate between history and theology, belonging properly in the former category. In many respects he has been growing more conservative in essentials since he began writing the book last October. The importance of archaeology [*latiore sensu*] has been impressing itself on him more and more; he has also been growing more conservative in dating various categories of literature, under the influence either of Eissfeldt or of Kaufmann.)—The second volume is an extremely useful collection of fifteen papers, some of great value, including the now inaccessible study on "Das formgeschichtliche Problem des Hexateuch" (1938). The date of individual studies extends from 1931 to 1955.

Bernard W. Anderson's *Understanding the Old Testament* (Englewood Cliffs, N. J., Prentice-Hall, 1957, xxiii + 551 pp.) is a clearly written and illustrated introductory text for colleges, seminaries, and groups. It is up to date and informed in both archaeology and critical literature, and may be highly recommended for serious students.

W. F. A.

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